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in 10. During the year 2 were discharged as recovered, 2 as relieved, 5 were transferred, and 8 died, the deaths all being from natural causes.

Reception Houses.

Up to the present the reception houses in Queensland have been merely resting places where patients from outlying districts could be temporarily detained until a sufficient number had been collected to be sent to one of the hospitals for the insane. Now, however, Dr. Ellerton proposes to submit certain amendments of the existing Lunacy Act whereby the reception houses will be done away with and sanctioning the admission of early cases of insanity into mental wards in general hospitals for a limited period on the authority of two medical certificates only, without any magistrates' or justices' orders.

Cost.

The net cost per patient per week at Goodna was 8s. 2d., at Toowoomba 8s. 3d., and at Ipswich 6s. 4d., or an average of 7s. 7d. per week.

Straits Settlements.

[FROM OUR SPECIAL CORRESPONDENT.]

THE WORK OF THE MEDICAL DEPARTMENT.

THE Principal Civil Medical Officer, Dr. W. Gilmore Ellis—who succeeded Dr. McDowell—has just issued his first annual report. It covers the year 1909.

The mean population, which for 1901, calculated from the census returns, was 572,249, is estimated at 636,691 for 1909. The estimated increase is based entirely on the increase between the census figures of 1891 and 1901, the better means of estimating from the number of births and deaths and immigration and emigration not being practicable as records for the latter are not reliable.

The birth-rate has declined in all the Settlements, being 26.69 per 1,000 as against 28.75 in the previous year. The death-rate is the lowest registered since 1899; in 1908 it was 46.78 per 1,000; in 1909 it was only 38.56. Of the number of deaths registered in 1909, 4,714 were among infants under 1 year of age, which represents a percentage of 19.7 of the total. It was hoped that the Government would bring in a Midwives Bill during the year, but nothing was done in that direction. During the year, however, some native women were being trained in the maternity wards of the General Hospital. These wards are slowly increasing in favour amongst the Chinese, and with more cases it is hoped the work will progress satisfactorily and bear fruit in the future.

There were fewer cases of plague during 1909 in the several Settlements, only 5 cases occurred, all of which were fatal. Cholera also showed a marked decrease—84 cases with 76 deaths, as against 198 with 178 deaths during 1908.

Beri-beri still claimed a number of victims, no less than 1,506 persons dying from that disease in 1909 out of a total of 2,118 cases. The results from the use of parboiled rice continue to give satisfaction, and the native patients at the several hospitals are exclusively fed on cured rice. Work on the etiology of this disease has been in progress throughout the year at the Research Institute, Kuala Lumpur, under Dr. Fraser. Dr. Ellis remarks that a dispatch had been sent through the High Commissioner to the Secretary of State transmitting a report recording the results. In the present state of knowledge, however, it has not been found feasible to advise Government as to any legislative measures of a preventive nature, although the results of work at many different laboratories seem to promise that this must soon be possible.

As in other parts of the world, the death-rate from phthisis is high, no less than 2,659 deaths being registered. This serious mortality, the report states, is likely to continue until some practical measures are put in force in the larger towns to lessen over-

crowding, improve ventilation, and remove advanced cases.

The number of cases of ankylostomiasis amounted to 335, compared with 192 in 1908, 80 cases being fatal. Over 30,000 cases were treated in the various hospitals throughout the Settlements, with a percentage of 12.01 deaths to the total treated. The total number of vaccinations performed throughout the colony in 1909 was 12,854.

In connexion with the Medical School there are now 72 full-course students, compared with 58 in 1908, studying for the L.M.S. diploma, of whom 17 are in their fifth year.

Much good work is being done in connexion with the pathological department of the colony.

Correspondence.

PARATYPHOID FEVER.

SIR,—Permit me, as one familiar with the scientific work being done in India by officers of the Royal Army Medical Corps and also conversant with the statistical facts relating to both the European and Indian armies, to enter a plea for clear thinking as to the use of the term "paratyphoid fever." From recent literature on this subject there appears to be a definite tendency at home to apply the term "paratyphoid fever" to cases of infection by the micro-organism known as *Bacillus paratyphosus* B. Concurrent with this, there is a tendency to overlook the infection by the closely allied micro-organism known as *Bacillus paratyphosus* A. This latter attitude may be due to the apparent infrequency of paratyphoid A infection in England or Europe. Before the nomenclature becomes crystallized and passes into general use by the profession, in the sense suggested by the recent literature published, it would be as well for the profession to realize that, in India and also in South Africa, there is recognizable a definite clinical entity caused by the *Bacillus paratyphosus* A. This clinical entity is a fever or pyrexia of the continued type. This view is worth bearing in mind, that it is to this continued fever, often of comparatively short duration and marked by a low case-mortality rate, caused by the *Bacillus paratyphosus* A, that the term "paratyphoid fever" should be applied, and applied only, at least for the present. Practically all the Indian cases are instances of infection by the A bacillus, and so far no case is diagnosed in the army unless the incriminating micro-organism has been isolated from the case.

Without going into the question of what is meant by a *Bacillus paratyphosus* B, or discussing how confusion has arisen, or the methods by which confusion can be avoided, as to whether a given micro-organism is *Bacillus paratyphosus* B or *Bacillus suispestifer*, it appears desirable to recognize that infections by these micro-organisms are really not "fevers" as ordinarily understood, but rather explosive infections of the nature of an acute enteritis, commonly the outcome of what is called "food poisoning." As explained, the infection by the A bacillus conforms to quite a different type, and is best described as "paratyphoid fever." From this point of view, it seems desirable that a confusion of ideas be avoided and that, failing better terms of expression, we limit the use of the expression "paratyphoid fever" to cases of fever in which the causative micro-organism is the *Bacillus paratyphosus* A, and describe as "food poisoning" those in which the causative micro-organism is either the *Bacillus paratyphosus* B, or the *Bacillus suispestifer*, or the other closely allied organism, the *Bacillus enteritidis* of Gaertner.

It must be admitted that we have still much to learn regarding all these infections, and subsequent investigation may compel us to modify our conceptions as to the whole group, but speaking in the light of our present knowledge, it is submitted that the views here outlined afford the safer and the wiser attitude to adopt. A considerable mass of evidence is accumulating, and much knowledge is being acquired in India as to the prevalence, symptoms, epidemiology, and identity of the causative micro-organism of paratyphoid fever. In fact—may I be pardoned for saying so—there is a small group of officers of the Royal Army Medical Corps working in India who probably know more



about infection by the *Bacillus paratyphosus* A (paratyphoid fever) than any one in Europe. We are hopeful that when their work is made public the present ambiguities may be removed.—I am, etc.,

Simla, May 4th.

R. H. FIRTH, Col., R.A.M.C.

THE PREVENTION OF MALARIA.

SIR,—In your interesting and impartial review (May 13th) of the *Prevention of Malaria*, by Professor Ross, C.B., you suggest that a school is "arising in India" which, although "not openly hostile," nevertheless would oppose the views advanced by that authority. Your reviewer has probably assumed that, as a result of prolonged experience in that country, there is a tendency to adopt opinions that are not held in other parts of the world. Deductions formed under such circumstances would, doubtless, be of value; and due weight would be given them by sanitarians. But it happens that this attitude is founded upon, and dates from, no better practical experience than that afforded by the haphazard and incomplete experiments of 1902-3 at Mian Mir, the financially starved efforts of 1904, and the "not openly hostile" opposition which has followed their outspoken criticism by Professor Ross. In the interval no practical effort has been made which would justify the medical profession in India in relying "for the country at large" solely upon quinine prophylaxis.

The recent appearance of increased vitality in this policy is in reality due to the scheme by which "The Committee for the Study of Malaria in India" (they having modestly renounced the title of the "Scientific Committee" with which they were formerly credited) have been endowed by the Government of India with the power to "direct and co-ordinate" investigations as to malaria, throughout the one and three-quarter millions of square miles of India. As this decision was communicated by the Government of India at the Antimalaria Conference in 1909, the natural inference has been that the members proposed this autocratic method of administration, and endorsed the peculiar views of the officers already selected for the purpose. The conference was, however, given no option in the matter. Sanitary Commissioners with Local Governments being thus thrust aside, and the helm placed in the hands of a committee with a predetermined policy, their diplomatic skill would be weak indeed if they were not able to guide the sanitary ship in the route they would desire. But consultation with all medical representatives of Local Governments at this conference left no doubt in my mind that professional opinion in India—as elsewhere—whilst recognizing quinine prophylaxis as a desirable adjuvant, where appropriate and practicable, would rely upon minor and major works directed towards "mosquito reduction" as of prime importance where permanent results are desired.—I am, etc.,

W. G. KING,

Colonel, I.M.S. (ret.).

Uffculme, N. Devon, May 15th.

CARDIO-SPLANCHNIC PHENOMENON OF ABRAMS.

SIR,—Dr. Gordon¹ seeks to deny the existence of this phenomenon, and he proceeds to fortify his contention by percussion of the right heart border, and the theoretic observation that the posture of the heart must be influenced by gravity.

The phenomenon, in brief,² is as follows: If the lower sternal region—that is, the part of the sternum contiguous to the heart—is first percussed in the erect posture, the sound elicited is one of resonance or hyper-resonance, but when the patient assumes the recumbent position a dullness supplants the resonance. This is the attitudinal cardio-splanchnic phenomenon. It is present in health, but absent when the splanchnic vasomotor mechanism is defective.

In describing the phenomenon the undersigned emphasized the importance of employing palpatory percussion.

The determination of the right heart border is neither a positive nor a negative evidence of this phenomenon. Percussion of the right heart border is difficult, and the results are often in accord with the prejudiced preconception of the observer. Again, by the time the right border is percussed the phenomenon has disappeared (*it is very limited in duration*), for the reason that a vigorous right ventricle

immediately disposes of the augmented quantity of blood which has been conveyed to it by the change in posture.

The writer has sought to simplify the percussion of the heart by a recently described method,³ and if Dr. Gordon will execute this method he will find decided discrepancies in his results, although the delimitation of the heart has absolutely no bearing on the phenomenon.

The latter fact is emphasized in my original contribution by the following quotation:

I have endeavoured by means of the Roentgen rays to determine whether any of the manoeuvres already suggested for provoking the cardio-splanchnic phenomenon in any way influence the cardiac diameters, and my investigations show that they do not.

As for the contention that the heart must be influenced by gravity, my skiagraphic findings to the contrary permit me to say that the situs of the heart is maintained by pulmonary suction.—I am, etc.,

San Francisco, May 5th.

ALBERT ABRAMS.

CONGENITAL APHASIA IN CHILDREN.

SIR,—The descriptions of defective children in Miss McCall's article, under the heading of "Two Cases of Congenital Aphasia in Children" (in the *JOURNAL* for May 13th, p. 1105), are no doubt of interest. The first case, however, is not one of aphasia, congenital or otherwise, neither is the other if the epithet of "word-deafness" applied to it is correct. Both are examples of defect in the receptive centres—in one case for written and in the other for phonated words—and to such conditions the term "aphasia," carrying with it the significance of imperfection in some part of the central neuromotor apparatus for language, is quite inapplicable. Congenital aphasia is distinctly rare and is apt to be confused with deaf-mutism by the unwary; congenital word-blindness, on the other hand, is a condition the existence of which even school teachers, who are as a class perhaps among the most conservative and unresponsive section of the community, are beginning to realize.—I am, etc.,

Renfrew, May 15th.

W. J. RUTHERFORD, M.D.

ENUCLEATION OF THE TONSIL.

SIR,—I would be glad of the opportunity of making some remarks on the discussion that took place at the last Annual Meeting on the anatomy of the tonsil.⁴

I wish to thank Dr. William Hill for remembering my papers sent to the two previous meetings, in which the claim was made that the so-called supratonsillar fossa was absolutely an intratonsillar space. The reason for the delay in this letter is that I have been trying to find reference by Mr. Seccombe Hett in the *Journal of Anatomy*, in which he stated that he had drawn attention to the intratonsillar position of the fossa. I have been unable to get access to it, but I am almost sure that it was subsequent to my first, if not my second, paper, as Dr. Hill says, "no one had been willing to give Mr. Hardie Neil the slightest support." In fact, the punishment meted out to me by Dr. D. R. Paterson was very severe, but, in the light of the now universal recognition of the truth of my contention, it contained a fine compliment—namely: "He traverses in detail views now generally accepted and sufficiently established to have found their way into most modern textbooks." A perusal of his letter (October 10th, 1908) will give a clue to the methods by which he found "that there could be no question of its [the supratonsillar fossa] being outside the tonsil." His researches could not, surely, have been done on either human or mammalian tonsils. His denial of the existence of the capsule is simply inexplicable. However, the newer textbooks will do the subject justice, and consign the statements about the supratonsillar fossa going beyond the tonsil up into the soft palate and down outside the tonsil to the level of the angle of the jaw, to the oblivion suitable to them. Those statements, I think, retarded the proper surgery of the tonsil for years. Photomicrographs of tonsils, fetal, adult, and animal, were sent with my paper in 1908 to the Annual Meeting. Some may be seen in the Royal College of Surgeons and Melbourne University.

As to the surgical anatomy of peritonsillar abscess, a

¹ BRITISH MEDICAL JOURNAL, March 25th, 1911.

² American Journal of the Medical Sciences, January, 1904.

³ La presse médicale, December 14th, 1910.

⁴ BRITISH MEDICAL JOURNAL, vol. ii, 1910, p. 1623.

ABRAMS